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## The Recognition and Reward of Employee Performance

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# The Recognition and Reward of Employee Performance

## Abstract

[Excerpt] This paper examines when and to what extent an individual's relative wage depends on his/her productivity relative to others doing the same job. Starting wages were influenced by background characteristics and training cost realizations but not by relative productivity. Wages one year later were influenced by productivity but the effects were small. The wage elasticity was .2 at small establishments and 0 at establishments with over 400 employees. The wage response to relative productivity and training costs was weaker in small labor markets, suggesting that wages do not fully respond to performance because of the firm specificity of job performance differentials.

## Keywords

wage, wage rate, secondary education, role, state government, American, student, performance, employment, school, role, employ, vocational, education, United States, youth, risk, work, job, training, occupation, college, examination, school, student, learning, economic, CAHRS, ILR, center, human resource, studies, advance

## Disciplines

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## Comments

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THE RECOGNITION AND REWARD OF  
EMPLOYEE PERFORMANCE

John Bishop

Working Paper # 89-05

CENTER FOR ADVANCED HUMAN RESOURCE STUDIES  
SCHOOL OF INDUSTRIAL AND LABOR RELATIONS  
CORNELL UNIVERSITY

This paper has not undergone formal review or approval of the faculty of the ILR School. It is intended to make the results of Center research, conferences and projects available to others interested in human resource management in preliminary form to encourage discussion and suggestions.

**The Recognition and Reward of  
Employee Performance**

Reprint 89-05

John H. Bishop

# The Recognition and Reward of Employee Performance

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This paper examines when and to what extent an individual's relative wage depends on his/her productivity relative to others doing the same job. Starting wages were influenced by background characteristics and training cost realizations but not by relative productivity. Wages one year later were influenced by productivity but the effects were small. The wage elasticity was .2 at small establishments and 0 at establishments with over 400 employees. The wage response to relative productivity and training costs was weaker in small labor markets, suggesting that wages do not fully respond to performance because of the firm specificity of job performance differentials.

Most hiring selections are based on very incomplete information. In part, this is a consequence of the remarkably small investment that most employers make in their hiring decisions. Small employers, for example, consider, on the average, only nine applications, interview only 4.5 applicants, and devote less than 10 hours of staff time to filling each position (Barron and Bishop 1985). Even more important is the notorious unreliability of most predictors of job performance such as the interview.

The paper is based on research that was funded by the U.S. Department of Labor and the U.S. Department of Education.

Mayfield (1964, p. 249) concludes a review of the literature with the statement, "The interview as normally conducted in a selection situation is of little value." Recipients of job offers are also poorly informed about many features of the job and about their alternative opportunities.

The poor quality of the information available when hiring decisions are made means that the terms agreed to may need to be changed if nonoptimal separations are to be avoided (Hashimoto and Yu 1980). Negotiation costs increase with tenure, however, because the individual develops firm-specific human capital and the rents to be divided grow in size. Consequently, formal renegotiation of employment contracts and its cousin, offer matching, are uncommon except for occupations such as sports, art, and research in which productivity is both highly visible and highly variable. More common are long-term contracts in which the more reliable party—the employer—promises to award wage increases and promotions on the basis of performance. This contract form is by no means universal, however, and, even where it prevails, there are often severe constraints on how key features of the contract, such as the wage, vary with measured performance. What are the optimal parameters of such contracts? How prevalent are such contracts? What are their terms? What is the effect of a worker's relative productivity on his or her relative wage? Which firms are likely to offer such contracts? These are the questions to be addressed in this paper.

The paper is organized as follows. Section I is a discussion of the factors that influence the optimality of contracts in which the employer awards bonuses or wage increases that depend on measures of job performance. Section II examines the degree to which employer perceptions of job performance influence relative wage rates. Estimates of the elasticity of relative wages with respect to relative productivity (as perceived by the wage setter) and how this elasticity varies by establishment size, unionization, and the size of the local labor market are presented. The final section summarizes the empirical findings, relates them to theory, and speculates on policy implications.

### **I. The Optimal Relation between Productivity and Wage Rates**

The widespread use of formal performance appraisal implies that most employers believe they can rate the productivity of their employees. Adjusting relative wage rates to reflect relative productivity produces three kinds of benefits for the firm. First, it serves as an incentive for greater effort. Second, it tends to attract to the firm more able workers and those who like to work hard (Freeman 1977). Third, it reduces the probability of losing the best performers to other firms and raises the probability that the least productive workers will leave. On the other hand, information about a worker's effort and productivity is often

costly to obtain, and the information asymmetries that these costs create often make it optimal to limit the adjustment of the wage rates to productivity. There are at least six reasons for this: (1) the inevitability of significant errors in measuring productivity, (2) variations over time in a worker's relative productivity, (3) productivity differentials that are either specific to the firm or not visible to other employers, (4) risk aversion, (5) deferred compensation of outstanding performance, and (6) other forms of reward for greater productivity.

### Measurement Costs

The first explanation is the high cost of accurately measuring a particular worker's productivity. In most jobs, an objective measure of productivity simply does not exist. This is part of the reason why, in November 1975, only 1.2% of the nation's workers were paid on a piece-rate basis and only 1.9% on a pure commission basis (Flaim 1979). In most work environments, productivity-based wage setting would have to use subjective evaluations by immediate supervisors. These supervisory assessments are known to contain measurement error. Meta-analyses of supervisor-rating studies have found that .6 is the upper bound on the correlation between the ratings given the same worker by two different raters (King, Hunter, and Schmidt 1980). Even more significant are the even lower correlations of supervisor ratings with carefully designed work sample measures of performance. The mean correlation is .42 in studies of civilian jobs and .27 in studies of jobs in the armed forces (Hunter 1983). If the purpose of merit pay is to forestall the loss of the most productive employees and the firm's measure of the productivity of individual workers is subject to error, the optimal wage-setting rule results in wages only partially adjusting to measured differences in productivity (Hashimoto and Yu 1980). If supervisory ratings and work samples are equally reliable and measurement error is random, an effort to set wages equal to expected marginal product would lead to a supervisory perception of a 10% productivity differential, resulting in a 4.2% wage differential.<sup>1</sup>

### Productivity Varies over Time

One of the reasons why productivity is difficult to measure is that it varies over time. The consistency of worker performance is greatest when conditions of work are stable, but, in the typical environment in which work environment is changing, correlations of output rates for

<sup>1</sup> Assume  $p^o = p' + u$ ,  $\text{cov}(p', u) = 0$ ,  $p^w = p' + v$ ,  $\text{cov}(p', v) = 0$ ,  $\text{cov}(u, v) = 0$ ,  $V(u) = V(v)$ , and  $r_{ow} = .42$ ; then, when  $p' = Bp^o$  is estimated,  $B = \text{var}(p') / \text{var}(p^o) = .42$ . If, instead, the purpose of merit pay is to provide an incentive for effort, if measurement error is uncorrelated with true productivity, and if workers are not risk averse, then the optimal wage-setting rule sets wage equal to measured productivity.

adjacent weeks range from .48 (Rothe and Nye 1961) to .69 (Rothe 1947), averaging .585 across four studies. If employers try to forestall the loss of their most productive employees by setting wage rates equal to the next-period expected productivity, the lack of performance consistency will result in an elasticity of future wage rates with respect to current productivity that is less than one.

The lack of performance consistency over time accounts for some of the differences between supervisor ratings of the same employee, so averages of past ratings are likely to be a better predictor of future performance than any single rating. Supervisor ratings, however, are influenced by previous ratings, so errors in measuring performance are not independent. Workers develop reputations (that may not be deserved) that influence later supervisors. Consequently, averaging ratings from many different years only moderately improves the employer's ability to predict the next-period true productivity.

### Differentials That Are Specific to the Firm

Third, productivity differentials between workers at a firm might reflect differences in skills that are specific to the firm or known only by the firm. If the worker is not able to translate high productivity with the current employer into a higher wage offer at another firm, the competitive pressure on the current employer to raise the individual's wage is reduced. Even if all productivity differentials within the firm reflect differences in generalized competence, it is very difficult in most jobs for other employers to measure these differentials accurately and, thus, base wage and job offers on them. Performance appraisals are almost invariably confidential (Lawler 1981). Most of the sources of information available to other employers are not reliable. Self-reports of productivity are properly treated with skepticism. References from past employers tend to be bland. Separating employees who have felt that they were unable to get a good job because of a poor recommendation have successfully sued their previous employer. This has made most employers reluctant to talk about their past employees. An illustration of this reluctance is provided by the following quote: "We warn our managers all the time. If someone calls you on the phone and asks you about someone who has left the company, you refer them to personnel. You don't say word one to them. You could be put in the position where you are going to be in court some day" (personnel director, Nationwide Insurance, Columbus, Ohio, 1985).

Thus, a major share of the productivity differences between workers at the firm are either irrelevant to or invisible to other employers and, hence, are functionally specific to the firm. When forestalling turnover is the reason for offering bigger wage increases to more productive workers, the optimal response of within-firm wage differentials to measured differences in expected productivity specific to the firm is



equal to the worker's share of investments in specific human capital (Hashimoto and Yu 1980). In Bishop and Kang's (1984) model of wage determination, the worker's share is

$$\left[ 1 + \frac{(1 + r_{\text{firm}})\phi_0/\text{pr}(k)}{(1 + r_{\text{person}})\phi/\text{pr}(s)} \right]^{-1}, \quad (1)$$

where the required rates of return for the firm and the worker are, respectively,  $r_{\text{firm}}$  and  $r_{\text{person}}$ . The expression  $\phi_0/\text{pr}(k)$  is the proportionate change in the probability of not being dismissed per unit change in second-period wage rate;  $\phi/\text{pr}(s)$  is the proportionate change in the probability of not quitting per unit change in the wage. When quits are highly responsive to the wage and fires are not, wage differences reflect differences in specific productivity. When fires are more responsive to the wage than quits are, wages respond less to differences in specific productivity. These implications will be tested in the empirical work.

### Risk Aversion

A fourth reason why the contracts that govern the employment relationship may specify only partial adjustment of relative wages to relative productivity is worker risk aversion. Realized productivity is often influenced by random factors such as the territory, machine, coworker, or supervisor to which the worker is assigned. If wages vary dollar for dollar with measured productivity, the worker is forced to accept a great deal of risk. Most workers would like to avoid such risks, so the optimal contract is a compromise between full and zero incorporation of realized productivity into the wage. Exactly where the compromise is struck depends on the strength of worker risk aversion, the responsiveness of effort to reward, and the variance of the random element (Cheung 1969; Stiglitz 1974). If firms can monitor the worker's effort, worker risk aversion will induce firms to offer contracts in which pay is based primarily on effort rather than on realized output (Harris and Raviv 1979). This further reduces the dependence of wages on realized productivity. Risk aversion also reduces the dependence of pay on past productivity when effort is exogenous and the purpose of merit pay is the retention of the firm's most productive employees (Freeman 1977).

### Deferred Compensation

A fifth reason for expecting the immediate response of relative wage rates to relative productivity to be small is that extra compensation for outstanding performance is often deferred. A merit increase in year 1 raises wage rates in later years even if the outstanding performance of year 1 is not repeated. Consequently, for workers anticipating long

tenure (or expecting to retire on a defined benefit plan keyed to salary in the last few years), the present value of a merit increase is considerably greater than its first-year effect. If anticipated tenure is short, however, deferred rewards for outstanding performance will not be attractive to a worker. This implies that firms with high rates of turnover would find that bonuses provide a more effective motivation for effort than merit increases. Indeed, bonuses and commissions are common forms of compensation in such high-turnover occupations as selling automobiles and insurance. From the firm's point of view, the reason for deferring rewards is that it simultaneously (1) rewards effort in period 1, (2) discourages turnover of the best performers in all future periods, and (3) encourages the weak performers to leave voluntarily.

### Other Forms of Compensation

A sixth reason why differences in relative productivity may not be reflected in wages is that the firm is recognizing the greater productivity in other ways (e.g., praise, desirable job assignments, greater autonomy, and lower likelihoods of layoff). If, for instance, a worker's satisfaction with a job (utility) is influenced by his or her relative status in the firm or among coworkers as well as by the absolute level of the wage, a merit increase or promotion generates two kinds of benefits for the worker: a somewhat higher wage and a movement up the firm's status hierarchy (Frank 1984). The greater the perceived importance of relative status, the smaller are the wage increases necessary to motivate workers to put out maximum effort.

## II. The Effect of Relative Productivity on Relative Wage Rates: Empirical Findings

To what extent are differentials in productivity (relative to one's coworkers with the same amount of tenure) incorporated into relative wage rates? The U.S. Bureau of Labor Statistics (BLS) periodically asks employers to describe the method they employ in setting wages. Table 1 presents the results of BLS surveys between 1968 and 1970 of firms with more than 50 employees. More than half the plant workers and 14% of white-collar workers had their wages set by a system that took no account of differences in productivity. The others either had their pay set individually or were on a range-of-rates system in which wage increases were based fully or partially on merit (Cox 1971). These surveys, however, provide no measure of the magnitude of the wage-rate response to merit and also lack coverage of people working in smaller establishments.

The National Center for Research in Vocational Education Employer Survey conducted in late spring 1982 provides a unique data set for examining the determinants and the parameters of merit-based pay. It

**Table 1**  
**Method of Wage Determination in Establishments**  
**with 50 or More Employees**

|                                  | Plant Workers<br>(%) | Office Workers<br>(%) |
|----------------------------------|----------------------|-----------------------|
| Everyone paid same rate          | 36                   | 3                     |
| Range with progression based on: |                      |                       |
| Seniority                        | 16                   | 11                    |
| Merit                            | 9                    | 36                    |
| Both merit and seniority         | 12                   | 22                    |
| Pay set individually             | 13                   | 28                    |
| Individual incentive             | 10                   | 0                     |
| Group incentive                  | 4                    | 0                     |
| Total                            | 100                  | 100                   |

SOURCE.—U.S. Bureau of Labor Statistics area wage surveys in 85 urban areas between 1968 and 1970 (Cox 1971).

provides retrospectively longitudinal data on the wage rates, turnover, and reported productivity of a pair of new hires for the same (or a similar) job at 659 different firms. Most of the respondents were owners or managers of small firms who were quite familiar with the performance of each of the firm's employees. At larger firms, interviews were typically conducted with both the personnel director and a line supervisor. The personnel director provided information on the company and the background of two recently hired employees, and the supervisor provided data on the training costs and productivity of the two new employees. These data are described in greater detail in the Appendix. The first member of the pair of recently hired employees was obtained by asking the main respondent to select "the last new employee your company hired prior to August 1981 *regardless of whether* that person is still employed by your company." The second member of the pair was obtained by asking the employer to select "another employee you hired [within the past 2 years] for the same or similar position but with some prior vocational training." In the event that the first person selected had prior vocational training, the second person selected was not to have had such training. Except for the fact that the two new hires are selected to have different amounts of vocational training, this procedure results in a random selection of two workers hired for the same or a similar job.<sup>2</sup> Seventy percent of these workers were still at the firm at the time of the interview.

<sup>2</sup> Differences in vocational training account for very few of the differences in wage-rates, productivity and training costs, so the requirement that the two workers have different amounts of vocational training has only a minor effect on estimates of the variance of worker productivity and training costs.

Given the well-known difficulties of measuring productivity accurately, it would be surprising indeed if relative wages of workers with only a year or so of tenure at a firm completely reflected their productivity relative to their coworkers in the job. On the other hand, the benefits of merit-based pay—greater effort, self-selection of more productive workers, and retention of the better performers—are likely to be important enough to require some recognition of relative productivity in wage increase and turnover decisions. Consequently, the empirical work in this and later sections tests both whether relative productivity has a positive effect on relative wage rates and whether the elasticity of relative wage rates with respect to relative productivity is less than one.

The econometric framework for examining the extent to which wages reflect actual differences in productivity will now be presented. Assume that the  $i$ th worker's wage relative to the mean for the job is described by the following equation:

$$W_{ij} - W_j = b_1(P_{ij} - P_j) + b_2(T_{ij} - T_j) + B(X_{ij} - X_j) + u_{ij}, \quad (2)$$

where

- $W_{ij} - W_j$  = the deviation of the individual wage from the mean for workers with similar tenure;
- $P_{ij} - P_j$  = the deviation of the individual's productivity from the mean for workers with the same amount of tenure;
- $T_{ij} - T_j$  = the difference between the training individual  $i$  needs to perform satisfactorily and that needed by the typical new hire;
- $X_{ij} - X_j$  = a vector of differences in credentials, background characteristics, and tenure between the individual and the mean for other new hires; and
- $u_{ij}$  = individual specific error term.

Data are not available on the means ( $W_j$ ,  $P_j$ ,  $T_j$ , and  $X_j$ ), so equation (2) cannot be estimated. For many firms, however, there are data on two workers doing the same job, so the following equation for the difference between the wage rates of person 1 and person 2 can be estimated:

$$W_{1j} - W_{2j} = b_1(P_{1j} - P_{2j}) + b_2(T_{1j} - T_{2j}) + B(X_{1j} - X_{2j}) + u_{1j} - u_{2j}. \quad (3)$$

We expect the coefficient for productivity to be positive and the coefficient for training to be negative. The coefficient on training is expected to be negative because workers who get more than average amounts of training do so primarily because they are slow learners or because their previous training and experience is weak. Even where

additional training reflects a decision to prepare the worker for a more highly skilled job, it should have a negative effect on wages while the training is occurring because some of that training is general and the worker must pay for it. The second hypothesis is that firms only partially adjust their wage rate to observed productivity and training investment (e.g.,  $b_1$  will be less than one).

Two equations for wage rates, one for the logarithm of the starting wage rate and the other for the logarithm of the latest wage rate, were estimated. For most of the workers who were still at the firm, the latest wage is the wage rate at the time of the interview, which is generally about a year after they were hired. For the workers who had separated, the latest wage rate was the wage immediately preceding the separation. The sample was limited to pairs of individuals both of whom had stayed at the firm at least 3 months. The Appendix describes the data set employed in the analysis and the method by which training investment and reported productivity were measured.

A simple model relating relative wages to relative productivity and relative training time but not to background characteristics of the worker is presented in columns 1 and 3 of table 2. The starting wage is set before the new hire starts work, so one would not expect productivity realizations to have as strong an effect on starting wages as on current wages. This expectation is confirmed, for the elasticity of starting wage rates with respect to productivity is only .08, while the elasticity of latest wage rates with respect to productivity is .22.<sup>3</sup>

Some employers are, apparently, able to anticipate when a new hire will require extra training and to offer lower wage rates to new hires who require extra training. The response of the wage to training costs is small, however. A 100-hour increase in training during the first 3 months—the cost of which is equivalent to one-fifth of an experienced worker's potential productivity—reduces the new hire's starting and latest wages by only 3.4%. The small size of this response suggests that most of the training in the first 3 months is functionally specific to the firm or that most employers are unable to anticipate how much additional training an inexperienced worker is going to require.

The finding that the elasticity of relative wage rates with respect to relative productivity is significantly below one implies that wages for workers who have been at a firm for about a year only partially reflect person-to-person variations in reported productivity on the job. The person who provided these productivity reports was, in most cases, the owner or manager of a small establishment (70% had fewer than 50 employees) and was thus quite frequently the person who decides on

<sup>3</sup> The elasticities were calculated by assuming a 1% increase (at the mean) in each of the three productivity reports.

**Table 2**  
**Effect of Worker Productivity on Wage Rates**

|  | Starting Wage      |                      | Latest Wage       |                     |
|--|--------------------|----------------------|-------------------|---------------------|
|  | (1)                | (2)                  | (3)               | (4)                 |
| Training time (100s of hours)                    | -.033***<br>(3.09) | -.020**<br>(1.99)    | -.033**<br>(2.39) | -.024<br>(1.41)     |
| Productivity:                                    |                    |                      |                   |                     |
| Second week                                      | .146**<br>(2.48)   | .090<br>(1.47)       | .102<br>(1.32)    | .052<br>(.60)       |
| Third week to twelfth week                       | .026<br>(.32)      | -.006<br>(.10)       | -.011<br>(.10)    | -.008<br>(.08)      |
| At interview or separation                       | -.010<br>(.22)     | -.011<br>(.26)       | .215***<br>(3.69) | .211***<br>(3.56)   |
| Relevant experience                              | ...                | .0155***<br>(4.54)   | ...               | .0093*<br>(1.94)    |
| Relevant experience squared                      | ...                | -.00028***<br>(2.26) | ...               | -.00027<br>(1.55)   |
| Total experience                                 | ...                | .0080***<br>(4.10)   | ...               | .0074***<br>(2.68)  |
| Total experience squared                         | ...                | -.00020***<br>(3.61) | ...               | -.000141*<br>(1.81) |
| Years of schooling                               | ...                | .011***<br>(2.87)    | ...               | .012**<br>(2.23)    |
| Relevant vocational education                    | ...                | .040***<br>(3.39)    | ...               | .031*<br>(1.87)     |
| Private vocational education                     | ...                | .006<br>(.24)        | ...               | .026<br>(.67)       |
| Female   | ...                | -.039*<br>(1.85)     | ...               | -.026<br>(.87)      |
| Known to be Targeted Jobs<br>Tax Credit eligible | ...                | -.062<br>(1.66)      | ...               | -.164***<br>(3.09)  |
| Union referral                                   | ...                | .430***<br>(4.78)    | ...               | .125<br>(.98)       |
| Tenure (years)                                   | ...                | ...                  | .051***<br>(2.74) | .065<br>(3.48)      |
| Tenure squared                                   | ...                | ...                  | -.0022<br>(.74)   | -.004<br>(1.41)     |
| Observations ( <i>N</i> )                        | 470                | 456                  | 470               | 456                 |
| <i>R</i> <sup>2</sup>                            | .173               | .360                 | .238              | .302                |

NOTE.—This table is based on fixed-effects models that compare two new hires for the same or a similar job at a firm. Other variables in the model were whether the job was temporary, whether the individual was a student, hours worked per week, whether referred by a relative, and whether subsidized by a program other than Targeted Jobs Tax Credit. The model for starting wage contained date of hire and date of hire squared. Numbers in parentheses are *t*-statistics.

\* Significant at the 10% level (two sided).

\*\* Significant at the 5% level (two sided).

\*\*\* Significant at the 1% level (two sided).

the wage offer for each individual. The regression is therefore capturing the relation between the productivity of individual workers as perceived by the person setting wages and the wage rate that is offered and agreed to.

In the absence of good direct measures of worker productivity, the employer will probably use background characteristics as signals for

predicting the productivity of new employees. As the firm learns more about a worker, we would expect wage decisions to depend more on observed productivity and less on background characteristics. Background characteristics should continue to have some role in determining wage rates, however, because (1) errors in measuring productivity leave a role for other variables that are correlated with true productivity and (2) most productivity differentials are probably functionally specific to the firm, and background traits affect the worker's threat point (marketability elsewhere) in the bargaining over the division of the rents generated by a better-than-average match. Many of the signals normally available to those making the hiring decision—age, sex, education, and previous relevant work experience—are also included in the data. It was, therefore, possible to test these hypotheses by modeling differentials in starting and latest wage rates as a function of differentials in both the signals of productivity and the measures of actual productivity. The results are presented in columns 2 and 4 of table 2.

The worker's background characteristics have large and significant effects on both starting and latest relative wage rates even when observed productivity is controlled. Traits that signal general human capital such as total experience and years of schooling have roughly equal effects on both initial and later wage rates. Holding work experience in a relevant job constant, an additional 5 years of total experience raises both starting and later wage rates by 3.3%–3.5%, and an additional year of schooling raises relative wage rates by 1.1%–1.2%.

Traits that signal occupation- or industry-specific human capital tend to have a smaller effect on later wage rates than on starting wage rates. Holding realized productivity and total experience constant, 5 years of relevant work experience raise wage rates by 7% at the start but by only 4% at the time of the interview. Being a referral from a union has an extremely large effect on starting wages but a much smaller effect on current wages. Including background characteristics in the model raises the  $R^2$  of the starting wage model from .173 to .360. For the latest wage, the increment to  $R^2$  is smaller, from .209 to .302. The greater importance of background characteristics in the starting wage model suggests that the value of these signals (as predictors of productivity) diminishes as the firm learns more about its new employee.

What is the unique effect of observed productivity when background characteristics are controlled? The findings are that realized productivity has almost no effect on the starting wage when background is controlled but large and significant effects on wage rates after a year or so at the firm. In the starting wage model, none of the coefficients on realized productivity variables is statistically significant, and the implied elasticity is only .03. In the latest wage model, the effect of current productivity is large and statistically significant. The elasticity of wage rates with

respect to productivity is .19, marginally below the .22 of the simple model that excludes background characteristics but includes tenure. This means that, while starting wages are based on background characteristics and credentials, later wage rates increasingly depend on actual job performance. Workers are rewarded for doing a better-than-average job.

The conclusion that relative wage rates at interview or separation depend on realized productivity as well as worker characteristics is subject to challenge, however, if employers set wage rates on the basis of worker characteristics such as recommendations from previous employers and aptitude test scores that are not available to the researcher. If such information is available to the employer and it has a continuing effect on wages even after the new hire has been at the firm for a year, the productivity measures will tend to pick up the effects of these omitted worker characteristics, and the coefficients on current and lagged productivity will have a positive bias. I examined the presence of omitted variables in wage equations by jointly estimating the starting and latest wage equations using a seemingly unrelated regression technique. Evidence that some of the determinants of relative wage rates are not included in the models is provided by the fact that there is a positive correlation of .4 between the errors of the two equations. Any possible bias produced by an omitted characteristic, however, seems to be very small. For the latest wage, it is only the contemporaneously measured productivity variable that has a large positive effect on the wage, and actual productivity in the first 2 weeks and the next 10 weeks shows no significant impact. Also, in the starting wage model, actual productivity in the first 2 weeks has the largest positive effect, and productivity 1 year after being hired has a tiny, nonsignificant negative effect. This pattern of coefficients suggests that (1) stable omitted worker characteristics are not a significant source of bias for the coefficients on the productivity variables in the model of the latest wage and (2) wages adapt quickly, though not completely, to the realized productivity of the new worker.

#### Where Is Merit-Based Pay Most Prevalent?

One would not expect all firms to be equally able or inclined to adjust relative wage rates to the realized relative productivity of workers. Small establishments and nonunion establishments are expected to be more likely to base wage increases on a worker's productivity. Large establishments are less likely to use merit pay because productivity is more difficult to measure and skills are more firm specific than at small establishments. In most jobs, merit pay will have to be based on subjective judgments. This is not a severe problem in small firms in which the owner is very familiar with each worker's performance. In large establishments, merit wage increases must be based on the opinions of line supervisors, who may misperceive the criteria to be used. If a



union represents the workers, the ability and inclination of management to adjust wages to productivity is reduced even further. The greater specificity of skills in large firms arises partly from greater specialization of function and partly from the fact that large establishments face fewer competitors for labor (Oi 1982).

The effective specificity of skills also depends on the size of the local labor market. In a large labor market, skills are effectively more general. Workers have a greater range of choices, and quits are likely to be more responsive to relative wage rates than they are in small labor markets. Equation (1), which characterizes the sharing ratio when human capital is specific to the firm, implies that, when quits are very responsive to the individual's wage, employers are induced to pay higher wage rates to their more productive employees. Consequently, the interactions between market size and productivity are expected to have a positive sign, and the interactions between market size and training time are expected to have a negative sign.

These hypotheses were tested by interacting the productivities and training differentials between two workers with unionization, establishment size, and labor market size in models of the latest relative wage:

$$W_{1j} - W_{2j} = (b_1 + a_1 Z_j)(P_{1j} - P_{2j}) + (b_2 + a_2 Z_j)(T_{1j} - T_{2j}) + B(X_{1j} - X_{2j}) + u_{1j} - u_{2j}, \quad (4)$$

where  $Z_j$  is a vector of firm characteristics (deviated from sample mean),  $a_1$  and  $a_2$  are vectors of coefficients on interactions with firm characteristics,

$$E(P_{1j} - P_{2j}) = 0, \quad E(T_{1j} - T_{2j}) = 0.$$

The  $a_1$  coefficients on the size and unionization interactions were negative as anticipated, and the  $a_1$  coefficient on the size/productivity interaction was significantly negative (see table 3). At nonunion establishments with 17 employees, the elasticity of the wage with respect to productivity is .2. Though the coefficient on the unionization interaction is not statistically significant, its point estimate implies that a unionized firm of that size would have a wage elasticity with respect to productivity of .10. The results imply that the elasticity of the relative wage with respect to relative productivity will be .09 at a nonunion establishment with 100 employees, -.01 at a unionized establishment with 100 employees, and zero at a nonunion establishment with 400 employees. Clearly, the relative wage rates of different workers in the same job do not vary proportionately with their productivity. In medium-sized unionized establishments and large nonunion establishments, there does not seem to be any immediate response of relative wages to reported

**Table 3**  
**Effect of Worker Productivity on Wage Rates: Interactions with**  
**Unionization, Establishment, and Labor Market Size**

|   | Starting<br>Wage   | Latest<br>Wage     |
|---|--------------------|--------------------|
| Training time (100s of hours)   | -.029**<br>(2.72)  | -.022<br>(1.53)    |
| Productivity first 2 weeks  | .055<br>(1.24)     | .068<br>(1.00)     |
| Productivity (most recent)  | ...                | .208***<br>(4.38)  |
| Size times productivity   | -.005<br>(.16)     | -.080**<br>(2.57)  |
| Union times productivity  | .212<br>(1.32)     | -.115<br>(.52)     |
| Labor market size times productivity  | -.014<br>(.48)     | .093***<br>(3.32)  |
| Size times training   | -.006<br>(.80)     | .003<br>(.32)      |
| Labor market size times training  | -.019**<br>(2.21)  | -.026**<br>(2.27)  |
| Union referral  | .406***<br>(4.39)  | .139<br>(1.10)     |
| Relevant experience   | .0156***<br>(4.58) | .0101**<br>(2.12)  |
| Relevant experience squared (divided by 100)                                      | -.027***<br>(2.21) | -.026<br>(1.49)    |
| Total experience  | .0075***<br>(3.83) | .0070**<br>(2.53)  |
| Total experience squared (divided by 100)   | -.019***<br>(3.39) | -.013*<br>(1.70)   |
| Years of schooling  | .011***<br>(2.70)  | .010*<br>(1.85)    |
| Relevant vocational education   | .043***<br>(3.64)  | .032*<br>(1.92)    |
| Private vocational education  | .006<br>(.21)      | .027<br>(.73)      |
| Female  | -.039*<br>(1.88)   | -.030<br>(1.02)    |
| Known to be Targeted Jobs Tax Credit<br>eligible                                  | -.070*<br>(1.85)   | -.160***<br>(3.05) |
| Received Comprehensive Employment and<br>Training Act-On-the-Job Training subsidy | .007<br>(.19)      | .003<br>(.06)      |
| Tenure  | ...                | .069***<br>(3.98)  |
| Tenure squared (divided by 100)   | ...                | -.0037<br>(1.38)   |

NOTE.—This table is based on fixed-effects models that compare two new hires for the same or a similar job at the firm. Models were estimated using seemingly unrelated regression. Other variables in the model were whether the job was temporary, whether the individual was a student, and hours worked per week. The model for the latest wage also contained tenure and tenure squared. The model for starting wage contained date of hire and the date of hire squared. The weighted  $R^2$  for the system was 0.348, and the correlation between the residuals of the two equations was 0.40. In the starting wage model, size and unionization are interacted with productivity in the second week. In the latest wage model, interactions are with most recent productivity. Numbers in parentheses are  $t$ -statistics.

\* Significant at the 10% level (two sided).

\*\* Significant at the 5% level (two sided).

\*\*\* Significant at the 1% level (two sided).

relative productivity. Such establishments are underrepresented in this data set, so the mean elasticity of .19 derived from this sample exaggerates the true average short-run response of relative wage rates to reported relative productivity. The coefficients on the interaction between establishment size and training time are not significantly different from zero.

The interactions of labor market size with productivity and training have the expected sign in both the starting and the latest wage models. The training interaction is statistically significant in both models. Differentials in training costs have no effect on starting wage differentials in labor markets that are one-quarter of the sample median in size. A 100-hour increase in training costs lowers a worker's starting wage rate by 2.6% in labor markets of median size and by 5% in labor markets of four times the median size. Labor market size has a significant positive effect on the responsiveness of wages to productivity. The elasticity of latest wages with respect to productivity is .09 in labor markets that are one-quarter of the sample median in size and .31 in labor markets of four times the median size. These effects are large. Specifically, they are somewhat larger than the comparable establishment size interactions. This is surprising because labor market size interactions result only from skill specificity, while firm size interactions are due both to monitoring costs and to skill specificity. If labor market size and establishment size both increase proportionately, there is very little change in the responsiveness of wages to relative productivity. This suggests that increased skill specificity, not higher monitoring costs, is the primary reason why large firms are less likely to adjust wages to productivity.

### III. Summary

Important differentials between firms have been found in the prevalence and power of merit-based pay. While a worker's reported productivity relative to peers does have important and reasonably rapid effects on relative wage rates at small and medium-sized nonunion establishments, it has almost no effect at unionized establishments with more than 100 employees and at nonunion establishments with more than 400 employees. Monitoring costs are low and skill differentials more general at the small establishments, and, as a result, effort and productivity are rewarded by higher wage rates. At large establishments, monitoring costs are high and skill differentials more specific to the firm, so wages do not quickly respond to productivity.

The second major contribution of the paper is the finding that there are large differences in productivity between workers doing the same job and that relative wage rates respond positively, but incompletely, to these differentials. A recent review (Schmidt and Hunter 1983) of studies that contained direct measures of output for different workers doing the same job at the same firm found that the coefficient of variation of

worker productivity, while highly variable across jobs, has a mean of about 20% for jobs paid on an hourly or salaried basis. My estimates of the coefficient of variation of job incumbents was .18-.19. Since there are fixed costs to employing an individual (facilities, equipment, light, heat, and overhead functions such as hiring and payrolling), the coefficient of variation of net marginal product is likely to be even greater (Boudreau 1983; Klein, Spady, and Weiss 1983).

The paper presents evidence that wage-rate differentials between workers with about 1 year of tenure only partially reflect current and past differentials in productivity and that there is essentially no immediate response of relative wage rates to productivity in very large establishments. Whether compensation for greater productivity comes over a longer time frame or in other ways cannot be tested in these data. Examination of the effect of productivity on turnover is left to another paper. The inevitability of significant errors in measuring productivity (that cannot be eliminated by measuring it repeatedly), the functional specificity of many productivity differentials, and worker risk aversion create a strong a priori case (see Sec. I) that compensation for greater productivity is often only partial, even in a present discounted value sense, for long-tenure employees.

The third major contribution of the paper is that it challenges an assumption—that individual wages are equal to individual marginal products—that is a keystone of some important practical applications of economics to public decision making in the field of education and training such as growth accounting and social benefit/cost analysis. It would appear that this very strong assumption may need to be replaced by something weaker, such as the assumption that wages equal expected marginal product given the employer's incomplete information set, or, perhaps, by something weaker still, such as Jovanovich's assumption that wages are equal to the average marginal product of all workers with the same tenure. As a result, when training programs are being evaluated, the possibility must be entertained that their effects on productivity (and therefore their social benefits) may be different from their effects on earnings (the private benefits received by trainees).

## **Appendix**

### **Data and Measurement Issues**

This paper is based on data from a survey of 3,412 employers sponsored by the National Institute of Education (NIE) and the National Center for Research in Vocational Education (NCRVE) conducted between February and June 1982. The survey represented the second wave of a two-wave longitudinal survey of employers from selected geographic areas across the country.

The first wave was funded by the U.S. Department of Labor to collect data on area labor market effects of its Employment Opportunity Pilot Project (EOPP). The survey encompassed 10 EOPP pilot sites and 18 comparison sites selected for their similarity to the pilot sites. The survey design specified a strategy of oversampling firms with a relatively high proportion of low-wage workers.

The second wave attempted to interview all the respondents in the first-wave survey. About 70% of the original respondents completed surveys for the second wave. In the bulk of the sample, respondents were the owners or managers of the establishments. In large organizations, the primary respondent was the person in charge of hiring, generally the personnel officer. When primary respondents were unable to answer a question, they were asked if someone else in the organization would have the information, and that part of the interview was completed with this other official. Other respondents included comptrollers, wage and salary administrators, and line supervisors (for questions about a particular recent hire). Most of the respondents were owners or managers of small firms who were quite familiar with the performance of each of the firm's employees.

The paper analyzes data from a subsample of employers who gave information on two different recent hires for the same job. The 3,412 employers who received the full questionnaire were asked to select "the last new employee your company hired prior to August 1981 *regardless of whether* that person is still employed by your company." A total of 818 employers could not provide information for a recent new hire. Most of these firms were small organizations that had not hired anyone in recent memory. The employers that provided information on one new hire were asked to provide data on a second new hire in the same job but with contrasting amounts of vocational education. Of the 2,594 employers that provided data on one new hire, 1,511 had not hired anyone else *in that job* in the last 2 years, and 424 had not hired anyone with a different amount of vocational training for that position in the last 2 years. As a result, data are available for 659 pairs of individuals who have the same job at the same establishment. Missing data on specific questions used in the model further reduced the sample used for estimation to about 480. Most of the establishments from which paired data are available are small. Seventy percent have fewer than 50 employees, and only 12% have more than 200 employees.

Each employer surveyed was asked about the training provided to the two new employees, current and starting hourly wage rates and an average rate paid to workers with 2 years of experience, and the productivity of each new hire at various points in his or her tenure. A copy of the relevant portions of the questionnaire can be obtained from the author on request.

Data were obtained on the amount of time that is devoted to training new employees during their first 3 months. Separate questions were asked about training hours spent in formal training, informal training by management, informal training by coworkers, and watching others do the job.<sup>4</sup> For the sample of firms and jobs, the means for the typical worker were as follows: watching others do the job, 47.3 hours; formal training programs, 10.7 hours; informal training by management, 51.0 hours, and informal training by coworkers, 24.2 hours.

A training-time index was constructed that valued and then combined the time invested in training activities during the first 3 months on the job. The management staff member who provided formal and informal training was assumed to be paid 1.5 times the wage of a coworker, and the trainee's time was valued as equal to 0.8 hours of coworker training time. When supervisors and coworkers are giving informal training to a new employee, the trainee is almost invariably involved directly in a production activity. Employers report that, for informal training, the trainees are typically as productive while being trained as they are when working alone. Consequently, informal training is assumed to involve only the investment of the trainer's time. The training-time index is equal to 0.8 times the hours spent watching others do the job plus 1.8 times the hours in formal<sup>5</sup> training plus 1.5 times the hours in training by management plus hours in training by coworkers.<sup>6</sup> The arithmetic mean of this index is 124 hours, implying that the value of the time invested in training a typical new employee in the first 3 months is about 23% of the output that a coworker would produce in 3 months.

The survey asked the employer (or, in larger firms, the immediate supervisor) to report on productivity of each of the individuals hired after 2 weeks, after 12 weeks, and currently or just before the individual left the firm. The supervisor was asked to place a rating on a "scale of

<sup>4</sup> In a few cases, employers reported that more than 520 hours (13 weeks times 40 hours a week) had been devoted to a specific training activity during the first 3 months on the job. Although the new hire might have received training from more than one supervisor, it is unlikely that two trainers were simultaneously in one-on-one contact with the new hire. Consequently, the computer edit of these data changed all reports of more than 520 hours involved in a training activity to 520.

<sup>5</sup> The cost of the trainer was assumed to be two-thirds of the forgone productivity since formal training often involves more than one trainee. Thus  $1.8 = (2/3)1.5 + .8$ .

<sup>6</sup> The index was constructed under an assumption that the four training activities were mutually exclusive. This implies that, if the sum of the hours devoted to individual activities is greater than 520, a reporting error has occurred that overstates investment in training. In the few cases in which the sum of hours devoted to training exceeded 520, the training-time index was adjusted downward by the ratio of 520 to the sum of the hours reported for individual activities. This procedure reduces the mean of the index by about 10%.

zero to 100 where 100 equals the maximum productivity rating any of your employees in [the employee's] position can obtain and zero is absolutely no productivity by your employee." The mean values of these indexes of reported productivity were as follows: first 2 weeks, 49.0; next 10 weeks, 64.6; and current, 81.4.

The interview questions about the productivity of recently hired employees were intended to provide indicators of the relative productivity of one worker at different points in time or two different workers in the identical job. They do not attempt to measure productivity in any absolute sense and therefore are not comparable across firms or across jobs in a firm. The questions asking for a rating of the productivity of particular workers have remarkably low nonresponse rates. Only 4.4% of respondents asked about a particular new hire's productivity during the first 2 weeks responded that they did not know or refused to answer. Comparably defined nonresponse rates for other questions about the new hire were 8.2% for previous relevant experience, 3.2% for age, 6.7% for education, 8.6% for time spent in informal training by a supervisor, and 5.7% for a three-question sequence from which the starting wage rate is calculated. The low nonresponse rate implies that respondents felt that they were capable of making such judgments and augurs well for the quality of the data that result.

In the paper, it is assumed that these productivity indexes are proportional transformations of true productivity plus a random error. If employer reports of a worker's productivity are equal to an unknown constant times the worker's true marginal product plus a random error, percentage differences in cell means of the productivity index can be interpreted as unbiased estimators of percentage differences in true productivity. If the variations in the productivity scores assigned by supervisors exaggerate the proportionate variations in the true productivity, estimates of percentage effects of productivity on the wage will be biased downward. Errors in measurement will have the same effect. Even though it is possible for a worker's true productivity to be negative, the scale was defined as having a lower limit of zero. Floors and ceilings on a scale typically cause measurement errors to be negatively correlated with the true value. If this were the case, the result would be an understatement of percentage differences between the productivity of different categories of new hires, and the measurement error bias would be mitigated somewhat. In my view, this latter type of bias is more likely than the former.

The validity and conservatism of the proportionality assumption can be checked by comparing coefficients of variation of productivity in this and other data sets. The similarity of this estimate of the coefficient of variation for job incumbents (.18-.19) to Schmidt and Hunter's (1983) estimate is mentioned in the text. Questionnaire studies that ask super-

visors to give dollar estimates of the output of workers at the fifteenth, fiftieth, and eighty-fifth percentiles typically obtain similar estimates. MacManus et al.'s (1986) survey of 349 supervisors for eight different types of jobs (not paid on a piece-rate basis) for which productivity measurement was thought to be feasible obtained an average coefficient of variation of .26.

The fact that the employer is reporting on the past productivity of particular employees may generate biases in data. Some of these employees quit or were fired, and some were promoted. These events might influence a respondent's memory of how productive the worker was initially and in the weeks preceding a separation. If this occurs, it would magnify the relation between productivity and the wage rate and turnover outcomes. This would strengthen the paper's main conclusion that wage rates of workers with 1 year of tenure on the job only partially reflect productivity differentials within the work group.

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